XXIV. *Experiments and Obſervations on the inflammable Air breathed by various Animals.*

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PHILOSOPHERS believed, till lately, that inflammable air had the power of killing animals who breathed it. Dr. PRIESTLEY, to whom we are much indebted for many diſcoveries and obſervations relative to inflammable air, made in conſequence of Mr. CAVENDISH's excellent paper on that ſubject, aſſures us, that inflammable air cauſes the death of animals as readily as fixed air, and that animals die convulſed in it. The doctor adds, that water abſorbed about one quarter of the inflammable air ſhaken in it, after which a mouſe lived in it as long as it would have lived in an equal quantity of common air. This air breathed by the mouſe was ſtill inflammable, though not ſo much as before.

Mr. SHEELE, who has made various important obſervations in chemiſtry, on the contrary aſſerts, that inflammable air not only does not kill the animals who breathe it, but that it is even good and innocent air. He relates ſome experiments to which it ſeems that nothing can be oppoſed, and they appear to contradict Dr. PRIESTLEY's obſervations. Mr. SHEELE has breathed inflammable air contained in a bladder, without receiving any hurt.

Seeing then that the experiments of theſe celebrated perſons contradicted each other, I began to ſuſpect that they might poſſibly be all true; and that their ſo contradictory effects might be owing to ſome circumſtance not yet attended to.

In order to follow ſome method in my reſearches about a point ſo delicate, and which ſo nearly intereſts human life, I firſt of all thought of aſſuring myſelf, whether or no animals could breathe inflammable air with impunity, when the receivers that contained it were immerſed in quickſilver. To this end, I introduced inflammable air, extracted both from zinc and iron, by means of the vitriolic acid, into various tubes filled with quickſilver, in which the air entered pretty free from moiſture. I then introduced various birds into thoſe tubes, and obſerved that they died in a few minutes time, but without any apparent ſign of convulſions. Theſe experiments, having been often repeated, were conſtantly attended with the ſame event.

Being aſſured, beyond any doubt, that the inflammable air obtained from zinc or iron, and made to paſs through quickſilver, was fatal to animals; I next wiſhed to obſerve, whether it retained the ſame properties when it had paſſed through water; in which caſe the volatile ſulphurous acid, or other vapour, is abſorbed by the water; but, on trying the experiments, I found that the birds died under theſe circumſtances as under the others (though not quite ſo ſoon) ſhewing likewiſe ſome ſigns of convulſion. I introduced ſome of this ſame air that had paſſed through water into a glaſs tube full of quickſilver, by a method which makes the air loſe all its moiſture. The birds died in it in the ſame manner as when the experiment was tried upon water. In all theſe caſes the air after the animals had died in it was ſtill inflammable, nor did its exploding properties ſeem to have been at all diminiſhed.

The inflammable air extracted from zinc, and that extracted from iron, is fatal to animals even after it has been ſhaken in water for a minute's time, or ſomething longer. By ſhaking it a long time, it becomes in ſome meaſure reſpirable; but then it is decompoſed in a great meaſure, and becomes of another kind, although it ſtill preſerves the properties of being inflammable, but in a ſmaller degree,

Not only birds but alſo quadrupeds die in inflammable air (though not ſo ſoon) and ſhew ſome ſigns of being convulſed.

It ſeems very ſtrange, that Mr. SHEELE could breathe inflammable air with impunity, when animals obliged to breathe it were killed in a very ſhort time. Admitting his experiments to be true, there remains nothing to be ſaid, but that the inflammable air in which animals die does not occaſion death becauſe it is conveyed to the lungs, but becauſe it affects ſome other organs of the animal body expoſed to that air, and neceſſary to animal life. It is not impoſſible to occaſion death by affecting the very ſenſible nerves of the noſe; it being well known, that various liquors, as very concentrated volatile alkaly, &c. if they are inſpired through the noſe, immediately affect the ſenſes, and occaſion death if they continue to act upon the *pituitary* membrane.

In order, therefore, to try whether inflammable air killed, only becauſe it was inſpired through the noſe, I ſtopped very accurately the noſes of various birds with ſoft wax, and in this manner I introduced them into receivers full of inflammable air extracted from zinc, and from iron, through water. The birds died within a few ſeconds, that is, juſt as they did when their noſes were unſtopped. Quadrupeds were tried after the ſame manner, and the event was the ſame.

Having in this manner exploded this new hypotheſis, there remained one more, which ſeemed to ſuggeſt a probable reaſon (ſince ſome reaſon there muſt be) for Mr. SHEELE's experiments being attended with reſults ſo different from thoſe of other experimenters. When an animal is introduced into a veſſel of inflammable air, its whole body is expoſed to that air; and it is not yet known by philoſophers what diſorders that fluid may occaſion to the animal frame. It is true that none are obſerved to be produced by other noxious kind of air; but if it be conſidered, that the vapours of ſulphur make a great impreſſion upon frogs, even when thoſe animals do not breathe them, but have their *aſpera arteria* tied up, it will not ſeem impoſſible for the inflammable air, in ſome manner or other, to act upon the body of animals. It may, perhaps, hinder the perſpiration; it may inſinuate itſelf through the pores of the ſkin; in ſhort, its action upon the body ſeems probable till experiments evince the contrary.

I therefore endeavoured to force various four-footed animals to breathe the inflammable air through the mouth only, without immerging their whole bodies into it. I chiefly uſed bladders tied to their mouths, but ſometimes I alſo made uſe of tubes which entered immediately into the wind-pipe. In both caſes the animals died in a very ſhort time: hence it became evident, not only that the inflammable air is pernicious to animal life, but that it does not act on the *body* of an animal; for I kept ſome of them immerged in inflammable air, with the mouth only out of it, and did not perceive any effect hurtful to them.

It being in this manner aſcertained, that the inflammable air could not be breathed by animals with impunity, it ſtill remained to find out the cauſe of Mr. SHEELE's miſtake. I began therefore to breathe the inflammable air contained in bladders, after the manner of Mr. SHEELE. The inflammable air uſed in my experiments was extracted from zinc and from iron by the action of the vitriolic acid, and it was received into bladders that were dry in the inſide, but a little moiſt on the outſide. The quantity of air contained in each bladder was about eighty cubic inches. The air coming out of the mattraſs paſſed through about one inch of water before it went into the bladders. At firſt I breathed the inflammable air with a kind of fear; but finding that it occaſioned no painful impreſſion, I continued breathing it with courage as long as I could. I breathed in a bladder filled with it eleven times, beginning after a natural expiration. This air when taken out of the bladder, was ſtill inflammable, and being tried with the teſt of nitrous air it gave II−28, III+20.

Before I go farther I muſt explain the formula which I uſe to expreſs the diminution of reſpirable air, or air of other kind, when mixed with nitrous air. My method is as follows: I have a glaſs tube of about eighteen inches in length, and half an inch in diameter, cloſed at one end, and of a conſtant diameter throughout its whole length: this tube has a mark at every three inches, which marks or diviſions I call *meaſures*, and every inch is divided into twenty equal parts; ſo that every *meaſure* is divided into ſixty portions, which I call *parts*. Into this tube, by means of an inſtrument which meaſures always one conſtant quantity of air equal to one meaſure of the tube, I introduce two meaſures of reſpirable air and one meaſure of nitrous air, after which I meaſure the diminution; then I introduce a ſecond meaſure of nitrous air, and again meaſure the diminution. The whole meaſures I expreſs in Roman characters, and the

parts

parts of a meaſure I expreſs in common numbers; for inſtance, when I ſay II−16 and II+10, the firſt expreſſion means, that after having introduced into the tube two meaſures of common air, and one meaſure of nitrous air, the ſpace occupied by the mixture of theſe two airs was two meaſures − 16 parts, or 60ths of a meaſure: and the ſecond expreſſion ſhews that, after having introduced another meaſure of nitrous air, the ſpace occupied was two meaſures + 10 parts. The reaſon and particulars of this method will be given hereafter in a paper expreſsly written upon the method of determining the degree of the ſalubrity of the air by means of nitrous and inflammable air.

Having introduced eight cubic inches of common air into the ſame bladder, I breathed it as long as I could; beginning after a natural expiration as in the experiment above related. I breathed it thirty-four times ſucceſſively, and afterwards found it very much altered, ſo that it extinguiſhed a light many times ſucceſſively. An animal introduced into a veſſel of that air immediately gave ſigns of uneaſineſs: and the air being tried with the nitrous air gave II+20, III+15; whereas, before it had been breathed, it gave with the ſame nitrous air II−15, II+18.

This experiment ſhews, that the air which remained in the bladder in the firſt experiment was not ſo good as that breathed thirty-four times ſucceſſively. In order to make this experiment with more preciſion, I breathed eighty cubic inches of common air, introduced into the ſame bladder, only eleven times; beginning after a natural expiration. Then I examined this air with the nitrous air, and found that it gave II−13, III+28.

Hence it is plain, that the mixture of inflammable and pulmonary air breathed eleven times is much inferior to common air breathed an equal number of times; ſo that there can remain no doubt but that inflammable air is at leaſt worſe than common air.

Willing, however, to aſcertain this matter ſtill better, I tried to breathe it immediately through a large receiver, partly immerſed in water, and ſwimming in it, ſo that the air within the receiver was of the ſame elaſticity with the external air. For this experiment I made uſe alſo of a glaſs tube bended in two different directions.

The air contained in the receiver was about 250 cubic inches. In all the trials made in this manner, I was never able to breathe the inflammable air more than three times, and even at the ſecond inſpiration I felt a great oppreſſion. As theſe experiments can be depended upon, becauſe they were often and at different times repeated, there ſeems to be reaſon enough to ſuſpect, that the bladder might poſſibly alter the nature of inflammable air, and render it more fit for reſpiration, notwithſtanding that the mere contact of the bladder ſeemed not ſufficient to produce ſuch an effect, which is irreconcileable with other facts: yet ſome reaſon muſt certainly exiſt ſufficient to explain Mr. SHEELE's experiments, which directly prove that the inflammable air contained in bladders can be breathed with impunity.

When I breathed this air according to Mr. SHEELE's manner eleven times ſucceſſively, I not only breathed it without any inconvenience, but obſerved that the firſt inſpirations were even pleaſing; more ſo than when I breathed common air. I felt a facility of dilating the breaſt, as if the air was as light as that at the top of high mountains. I never felt a like ſenſation, even when I have breathed the pureſt dephlogiſticated air. I do not think that I was miſtaken in theſe ſenſations, or gave a looſe to imagination, becauſe I was rather prejudiced againſt the inflammable air, after I had ſeen various animals immediately die in it, and I was rather fearful when I firſt began to breathe it: beſides, this facility of breathing it, accompanied with a pleaſing ſenſation, I have conſtantly obſerved in all my experiments upon this ſubject.

This pleaſure, however, I paid very dear for in another experiment, in which I was near loſing my life. Having filled a bladder of the largeſt ſort with about 350 cubic inches of inflammable air extracted from iron filings through water, which air was not at all diminiſhed by the mixture of nitrous air; I began to breathe it boldly (owing to the encouragement received from the above related experiment), and reſolved to breathe it as long as my ſtrength would permit me, after having made a very violent expiration in order to evacuate the lungs of the atmoſpheric air. Having made the firſt inſpiration I felt a great oppreſſion upon my lungs. Towards the middle of the ſecond inſpiration I heard Mr. CAVALLO, who favoured me with his aſſiſtance in theſe experiments, ſay, that I was become very pale: by this time the objects appeared confuſed to my eyes. Notwithſtanding this, I made the third inſpiration; but now my ſtrength failing, I loſt my ſight intirely, and fell upon my knees. In this ſituation I breathed the air of the room, but my knees not being able to ſupport me, I fell intirely upon the floor. However, in a ſhort time I came to myſelf, ſo as to be able to get up, &c.; but my reſpiration continued to be affected with difficulty and pain, as if I had a great weight upon the breaſt; nor did I perfectly recover before the next day.

It muſt be obſerved, that during this experiment I kept my noſe cloſe ſtopped.

This ſame inflammable air contained in the bladder, which I had breathed three times, was examined in various manners, and was found to be as inflammable as before; it exploded as uſual, when mixed with dephlogiſticated air, but after having been ſhaken in water for a ſhort time, being tried with the nitrous air, it gave III – 10, IV – 10, whereas before it was not at all diminiſhed. At this time the common air, with the ſame nitrous air, gave II – 14, II + 10. Hence it appears, that the inflammable air, after being breathed, is rather better than before, becauſe in that caſe it is a little diminiſhed by the addition of nitrous air.

In order to aſcertain whether this alteration was occaſioned by the bladder or no, I made the following experiment, which, having been often repeated, was conſtantly attended with the ſame event. I introduced into a bladder, which was ſometimes moiſt and ſometimes dry, a quantity of inflammable air, extracted as well from zinc as from iron, through water, and having kept it in that ſituation for ſome minutes, beating in the mean while the bladder, to keep the air in agitation, I afterwards took it out, and by the mixture of nitrous air obſerved, that it ſuffered no diminution, exactly as it ſuffered none before it had been put into the bladder.

Having aſcertained, in this manner, that the bladders do not in any manner contribute to render the inflammable air extracted from metals better in its nature, there remained no other way of aſcertaining Mr. SHEELE's experiments, and of underſtanding why I had been able to breathe it eleven times, than by ſuppoſing that the air of the lungs, which can never be thoroughly emptied by being mixed with the inflammable air, alters it, &c. It is well known, that in an ordinary expiration about thirty-five cubic inches of air are expelled from the lungs. In a very violent expiration, following a natural inſpiration, the air expelled may amount to ſixty cubic inches. Theſe forty inches of pulmonary air are mixed with the inflammable air, and are expelled from the lungs in proportion to the remaining air that is breathed after that the lungs have been thoroughly emptied. In the expement above related, of the three inſpirations I made into the inflammable air, it may be eaſily ſuppoſed, that twenty inches or more of pulmonary air were joined with the inflammable air, and entered into the bladder. This pulmonary air, although it is itſelf partly phlogiſticated, is however diminiſhed by nitrous air; and when it ſtands in the bladder it is nearly equal to $\frac{1}{17}$th of the inflammable air of the bladder breathed three times; hence this loſt ten parts by the mixture of nitrous air.

This explanation, which it is neceſſary to adopt after having exploded all the other hypotheſes, is very analogous to the above related experiment of the ſmaller bladder filled with inflammable air which was breathed eleven times ſucceſſively. This air was breathed after a natural expiration, ſo that there ſtill remained in the lungs about ſeventy-five inches of common air. Theſe ſeventy-five inches of pulmonary air, together with the eighty inches of inflammable air, were mixed together during the eleven inſpirations and expirations; hence the air of the bladder was a mixture of nearly equal portions of inflammable and common air; and, accordingly, when tried with the nitrous air, it was found to be much better (though it had been breathed eleven times) than the air of the large bladder breathed three times only, after the lungs had been emptied as much as poſſible.

All the other experiments that I have made in confirmation of this hypotheſis ſeem univerſally to favour it. If a Guinea pig is introduced into a receiver containing 400 cubic inches of inflammable air, or a ſmall bird into only fifty inches of it, and they be left therein till they are dead, that air afterwards will not be ſenſibly diminiſhed by nitrous air; but if a much larger animal is introduced into the 400 inches of inflammable air, or a ſmall animal into a few cubic inches of that air, then it will be found to be ſenſibly diminiſhed by nitrous air; and this diminution will be greater as the animal is larger in proportion to the quantity of inflammable air. A larger animal imparts a greater quantity of its pulmonary air to the inflammable air; and the inflammable air will be found joined to a quantity of pulmonary air, which is ſo much the leſs as the animal is ſmaller.

Mr. SHEELE ſays, he found that the inflammable air after being breathed ſome time intirely loſes its inflammability; from whence he concludes, that the lungs, inſtead of imparting ſome phlogiſton to, imbibe it from, whatever ſubſtance it can be extracted. Though all the direct experiments which ſhew that a phlogiſtic principle is continually detached from the lungs, and joins itſelf to the common air, were wanting, ſtill Mr. SHEELE's conſequence could not be drawn, becauſe the experiment is not true. With reſpect to my own experience I may ſafely ſay, that I have always found it inflammable in every circumſtance, even after I had breathed it eleven times ſucceſſively: and I have not only found it inflammable in the bladder, but I have fired it in the act of letting it out of my mouth. In this manner a flame may be produced from the mouth, of various inches in length, and two or more inches in breadth.

But whence does that ſenſation of levity and facility of breathing the inflammable air, which I have deſcribed above, originate? At preſent I can only have recourſe to a mere mechanical cauſe for a ſolution, for I do not obſerve in inflammable air any property that ſeems capable of altering the lungs upon a chemical principle; neither have I obſerved any decompoſition of air, or alteration of the fluids of the animal. It has been obſerved, that inflammable air, after being breathed, comes out of the lungs with the ſame properties it had before. It is alſo known, that inflammable air is not ſenſibly abſorbed by water, at leaſt after a ſhort time. The lungs, or more properly the pulmonary veſicles, are continually moiſtened with fluids; but that air cannot be abſorbed by them, except it be firſt decompoſed. Nothing elſe therefore remains to which we can have recourſe for an explanation of the above mentioned ſenſations, but the well known levity of the inflammable air compared with common air. And indeed the ſenſation I felt when I breathed that air, is like that of a very light fluid which does not oppreſs the lungs, and is hardly felt. This explanation agrees exactly with ſome experiments I have made with common air rendered more light by fire. This air I have found may be breathed much eaſier, although not for ſo long a time, as when it is more condenſed. It muſt be ſaid, indeed, that this is occaſioned by another particular cauſe, which has nothing to do with the caſe of the inflammable air, and which cannot be properly examined in this place.

After all, it ſtill remains to be known, why inflammable air, which kills animals ſo ſoon, may be breathed without any oppreſſion, when in a ſmall quantity, *viz.* when it is mixed with common air; and the following experiments, which are very analogous to thoſe related above, will ſhew that the queſtion is not unintereſting.

I introduced 350 cubic inches of common air into a bladder, and after having made as ſtrong an expiration as I could, I applied the neck of the bladder to my mouth, and breathed the air it contained forty times ſucceſſively. Afterwards, having taken the air out of the bladder, I found that it extinguiſhed a light ſeveral times ſucceſſively. It formed various cryſtals with the oil of tartar, but after a very conſiderable time; ſome of theſe cryſtals had the ſhape of needles, others were like flowers: being tried with the nitrous air, it gave II–18, III+18. This air, therefore, was very much phlogiſticated, nor could I poſſibly have breathed it longer than I did, without falling upon the ground, as I already felt my ſtrength failing, and the objects appeared confuſed before my eyes. Into ten cubic inches of this air I introduced a ſmall bird, which, as ſoon as it began to breathe it, made various contorſions with its body, and ſeemed to ſuffer a great deal. It died in ten minutes time; whereas another little bird introduced into a like quantity, that is, into ten cubic inches of common air, lived in it fifty-two minutes, nor did it ſhew any ſign of uneaſineſs before it had been in five minutes.

It remains to be accounted for, why the bird could breathe for five minutes longer in the air of the bladder than a man could. It will be ſufficient to conſider, that when a man in this experiment has made the laſt expiration into the bladder, he is in a ſtate of pain, and his lungs are loaden with a ſuperfluous quantity of phlogiſton, which is not communicated to the air of the bladder; whereas nothing of this takes place with the bird, which, beſides its being in vigour, has a quantity of common air in its lungs. This ſeems confirmed by an experiment, which admits of no doubt. Having breathed the air of the bladder as long as I could, I ſtopped the neck of the bladder with my finger, then breathed the common air ſeveral times; and afterwards putting the neck of the ſame bladder to my mouth again, I breathed that very ſame air four times ſucceſſively. Now there is no doubt but that a bird could have breathed it much longer: the reaſon of which diverſity ſeems to be the following, *viz* that a ſmall bird is in want of a ſmall quantity of air for every time it breathes, whereas a man is in want of a much greater quantity; hence the air is rendered more eaſily noxious, and unfit for reſpiration. From all which it may be concluded, that we are in want of a certain quantity of common air neceſſary for reſpiration, and for the ſupport of life; and that this air, after being inſpired, comes out of the lungs leſs fit to be breathed a ſecond time.

It has been obſerved, that the inflammable air cannot be breathed when the lungs are emptied of common air as much as poſſible; but that it may be breathed when the lungs are in a natural ſtate, in which ſtate a quantity of common air, equal to about forty cubic inches, is known to exiſt in the lungs of an adult perſon. This pulmonary air is not infected ſo far as to be incapable of being breathed various times, and of ſupporting life. After having made a natural expiration I have with force expelled from my lungs about thirty inches of air into an empty bladder; and this pulmonary air I have generally been able to breathe eight times ſucceſſively, but never longer. It is true, however, that I breathed it with ſome oppreſſion, even from the beginning, which does not happen when the inflammable air contained in a bladder is breathed, the lungs being in a natural ſtate.

And now it ſeems no longer difficult to give an anſwer to the queſtion propoſed above, and to account for the ſmall difference obſerved in the breathing of the two different kinds of air in the bladders. The inflammable air, when joined to a great quantity of common air, may be breathed ſafely, becauſe there is a quantity of common air ſufficient for various inſpirations, and that the mixture of the two airs may be breathed till this common air is thoroughly infected. But the inflammable air itſelf is not altered nor decompoſed by the reſpiration. Wherefore we muſt conclude, that the inflammable air is not ſuch a kind of air as can by itſelf alone be directly uſeful for reſpiration. It muſt rather be conſidered as if there was nothing of that air in the caſe of the bladder; and indeed it is found by experience, that the pulmonary air itſelf may be breathed eight or nine times in an empty bladder. The not being able to breathe it eleven times ſucceſſively, as was done when there was inflammable air in the bladder, and the feeling an oppreſſion in the firſt caſe and not in the ſecond, muſt be intirely attributed to the want of thirty-five cubic inches of air expirated, which are neceſſary to give the lungs all the neceſſary expanſion; whereas, in the other caſe, the inflammable air ſerves to fill up ſpace, and, together with the common air, contributes to fill the lungs; ſo that the inflammable air, conſidered under theſe circumſtances, and under this point of view, may be ſaid to be uſeful for animal reſpiration. This explanation ſeems moſt evidently demonſtrated by the following experiment. If thirty-five cubic inches of common air are introduced into the bladder, and this air be breathed when the lungs are in a natural ſtate, it will be found, that one may breathe it for twenty times or longer; whereas, when the bladder was empty, it could not be breathed more than nine times at moſt.

Before I finiſh this paper it will be proper to mention another cauſe, which, perhaps, alſo contributes to render the inflammable air of the bladder leſs noxious: this is the levity of the inflammable air itſelf with reſpect to common air, which hinders the inflammable mixing with the common air. The inflammable air ſwims continually upon the common air, juſt as æther ſwims upon water; and the inflammable air ſwims ſtill better than æther, becauſe it is much lighter in compariſon than æther. Various experiments made upon volatile ſubſtances have convinced me of this truth. If equal quantities of common and inflammable air, or dephlogiſticated and inflammable air, are put into a tube, and two birds are introduced in it, ſo that one of them may ſtand at the top, and the other at the lower part of the inverted jar; it will be found, that the firſt mentioned of theſe birds not only will die conſiderably ſooner than the other, but will ſhew ſigns of uneaſineſs as ſoon as it is come to that place.

The inflammable air, therefore, when breathed together with a conſiderable quantity of common air, muſt always ſwim at the top of it, filling the cavity of the wind-pipe, &c. while the common air occupies the lower place, and filling the ſmalleſt pulmonary veſicles is ſubſervient to the ordinary functions of the lungs.

Here I put an end to my obſervations upon inflammable air conſidered with reſpect to reſpiration; but I beg leave to add a few words reſpecting a property of the inflammable air, which, as far as I know, has been overlooked by the moſt diligent obſervers.

I mean here to ſpeak of ſuch inflammable air as is extracted from metals, by means of oil of vitriol, eſpecially that extracted from iron and zinc. The air of theſe metals, when preſented to the flame of a candle, not only burns with a whitiſh flame inclining to green (as is well known); but exhibits a kind of ſparks or exploſions which may be eaſily diſtinguiſhed between the body of the flame by their vivid light. Theſe ſparks, which are of a vivid colour, dart in every direction. They might be eaſily taken for thoſe ſparks that are emitted from red-hot iron; or they might be compared to very ſmall grains of gunpowder, if theſe were inflamed ſucceſſively, and without ſmoke; or they might even be compared to charcoal that ſparkles, but without any noiſe. This phenomenon ſeems very intereſting, as it reſpects the nature of the inflammable air itſelf. What ſeems to me moſt ſingular is, that this appearance forms a diſtinctive character between the inflammable air of metals, and that extracted from animal or vegetable ſubſtances; at leaſt I may ſafely ſay, that I never found the inflammable air of animal or vegetable ſubſtances ſparkle like that extracted from metals. In ſeveral of the former kinds of air I could obſerve no ſparkling at all; in others the ſparks were ſo few that they might be conſidered as nothing in compariſon to the ſparkling of the inflammable air from metals.

The inflammable air of metals itſelf, if left in contact with water for a long time, or ſhook in it till it becomes leſs inflammable, will in great meaſure loſe its ſparkling property, and at laſt loſes it intirely, when it is become in a ſtate of being hardly inflammable. I have obſerved,

that

that the inflammable air is the more difficult to be decompoſed, by being ſhaken in water, as the number of the ſparks it ſhews when burning is greater; and according to this number of ſparks, the inflammable air makes greater or weaker exploſions when mixed with the dephlogiſticated air; ſo that it ſeems proved by experiments, that the phlogiſtic principle is more fixed and in greater quantity combined with the inflammable air of metals, than with that of vegetable or animal ſubſtances. I do not mean to deny the poſſibility of finding other ſpecies of inflammable air extracted from other ſubſtances beſides metals which may explode like that extracted from metals; but I only ſay, that in thoſe caſes the inflammable air will alſo ſparkle more, and will be found leſs eaſy to be decompoſed by water. There are other ſubſtances that give the inflammable air in great quantity, and which cannot be conſidered as animal or vegetable ſubſtances, but come rather near the nature of metals; as, for inſtance, the ſpathoſe iron, from which I extract a good deal of inflammable air by the action of fire only, applied to a mattraſs. But the metal in this ſubſtance is not in its pure ſtate, and it may be conſidered rather as a calx of iron than true iron. Accordingly, this air can hardly ſparkle at all; it explodes more like the inflammable air of vegetable or animal bodies than that of metals, and it is eaſily decompoſed in water.

This property of inflammable air of metals which I have diſcovered, throws great light upon the analyſis of the decompoſition of that air which I have made in two different ways. The firſt is to fire it together with common or dephlogiſticated air, in veſſels filled with very pure quickſilver, and alſo in veſſels filled with diſtilled water. The ſecond method is to decompoſe it by ſhaking it in pure diſtilled water. In the firſt proceſs a great number of experiments are required in order to obtain a ſenſible reſiduum; beſides, the igneous part is loſt. The ſecond method requires an exceedingly long time, but it is the moſt complete; for which reaſon I have uſed it for the decompoſition of other kinds of air.